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JOURNAL
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A MASS ESP TEST USING TELEVISION¹

BY D. MICHIE AND D. J. WEST

INTRODUCTION

IN 1926 Sir Oliver Lodge and Dr V. J. Woolley (1) utilized a radio audience as participants in a telepathy test. Various attempts have since been made to employ broadcasting for mass ESP trials, but this is the first occasion that the medium of television has been used for the purpose. It has the advantage of establishing a closer link between the conductor of the test and the participants than would be expected from the sound of the voice alone.

The possibilities for ESP research provided by broadcasting might have been used more often but for a technical difficulty. When the target series in a guessing experiment is short, as it has to be owing to the time limitations of a broadcast, and when large numbers of participants are guessing at the same set of targets, the ordinary methods of statistical assessment cease to be applicable. The difficulty arises because, quite apart from the influence of telepathy, a substantial portion of the participants may display similar guessing habits and sequence preferences. The difficulty can best be illustrated by an artificially simplified illustration.

Suppose an experimenter in the studio tosses a penny (a theoretical penny that is completely unbiassed) and asks the viewers to guess the result—heads or tails. Suppose that he does this three times and then has to stop, that the actual target series so obtained is Heads, Tails, Heads, and that 300 viewers send in their guesses.

¹ D.J.W. was responsible for the experimental, and D.M. for the statistical, part of this work. D.J.W. took part in the work by courtesy of the Parapsychology Foundation.

If one could assume that in the absence of a telepathic influence the viewers would on each occasion choose between heads and tails completely at random, half of them on average guessing heads and the other half guessing tails, the expected outcome would be obvious. At each toss of the coin there would be an expectation of 150 out of 300 right guesses. For all three trials, out of a total of 900 guesses, chance expectation would be 450.

These assumptions are false. In the first place, more people are likely to call heads than tails. Since the particular target series had more heads than tails, this simple preference would increase the proportion of correct guesses above 50%. This particular difficulty could be overcome by ensuring an equal proportion of heads and tails in the target series, but there are also sequence and position preferences to be reckoned with. For instance, there might be a strong tendency to call heads as a first guess. In addition there might be a resistance to making the same guess twice, so that a call of heads would tend to be followed by a call of tails. Since the particular target series begins with this 'favourite' sequence, one might still find more than 50% of the guesses correct even if the target series were lengthened to provide an equal number of heads and tails.

Supposing that it could be arranged that every possible sequence was represented an equal number of times in the target series, the difficulty would remain, for sequence preferences might vary according to position in the series. The heads-tails sequence preference would probably be strongest at the beginning and tend to wear off or even be reversed later in the series. It is this unknown combination of sequence and position preferences that makes the difficulty. The example given is an extreme case. In actual experiments the effects are more subtle and unlikely to produce spectacular deviations. But since the ESP factor, as it manifests in mass tests, is itself a marginal phenomenon, it is essential to eliminate all trace of spurious effects.

The idea underlying the method of evaluation applied in this experiment is to obtain an empirical estimate of the extent of the effect of sequence and position habits in the guesses so that it can be allowed for. If the method is valid, it opens the way to a better utilization of the data from mass ESP tests.

The result of the present experiment was that the participants' scores taken as a whole showed no evidence of ESP. But one individual subject, Mr B. Downey, produced a score which was considered sufficiently suggestive to warrant following up. In the subsequent tests which were accordingly performed on him, this individual gave outstanding evidence of ESP ability.

THE CONDUCT OF THE MASS TEST

The experiment took place on the evening of Thursday April 19th, 1956, in the course of a popular science feature run by Science Television Services Ltd. All those concerned with the programme deserve most grateful acknowledgement for their cheerful compliance with our many troublesome demands as regards screening of the agent, signalling for the guesses and timing the experiment. If the instructions to viewers were not as detailed as they might have been, and if the experiment itself was a trifle hurried, so that the agent, Mr G. W. Fisk, was somewhat distracted by the effort to keep in step with the signals, these imperfections must be excused in a first experiment with an unfamiliar medium and a limited time for rehearsal.

On the evening before the transmission, D. J. West prepared 18 different sets of targets all conforming to D. Michie's specifications, which will be described later in the report. He enclosed the 18 lists in separate envelopes and asked an assistant (who took no further part in the experiment) to place the 18 envelopes inside 18 outer envelopes so that D. J. West did not know which was which. A few moments before the transmission began D. J. West proffered the 18 envelopes to Mr Fisk, who picked out one for use as the definitive target series.

Prior to the transmission an announcement had appeared in the *Television Times* which included the following:

'The viewers on Thursday will have three pictures before them on the TV screen—a wheelbarrow, a trumpet and a canoe. The agent, hidden by a screen, will concentrate on each of them in turn in a random order, while the viewers will be asked to mark on the coupon on this page the pictures they think he is passing on to them by "thought transference".

'The viewers will then be asked to send their coupons to. . .'

The coupon mentioned contained spaces for twenty guesses between the three possibilities of wheelbarrow, trumpet and canoe. It also contained spaces for recording name, address, sex and whether 'I feel I have scored $\frac{\text{well}}{\text{poorly}}$ '.

In the programme itself the experiment was introduced by Dr Carthy, who reminded viewers to have their pencils and coupons ready. First Mrs Kovaleska, a lady who had a personal psychic experience to recount, told her story. Then Dr Carthy introduced D. J. West to the viewers and asked him to comment. In doing so he emphasised the need for experiment, and the test forthwith began. G. W. Fisk appeared and explained that he was going to

retire behind a screen and look at a series of picture cards, canoe, wheelbarrow and trumpet. A voice would call out 'one', 'two', 'three', . . . and he would look at the pictures one by one keeping in time with the voice. Viewers were asked to register their guesses simultaneously, either using the coupon in the *Television Times* or else putting them down on any old scrap of paper and copying them on to the proper form later.

As he finished speaking, G. W. Fisk quickly stepped behind a screen, which was arranged in a corner so that he could look at the cards out of sight of the other people in the studio. An assistant went behind the screen with him to arrange his headphones so that he would hear the voice calling out the numbers. It was intended to call the numbers at five-second intervals, so as to leave ample time for each guess, but in the event it seemed as if the series was run through faster than planned, although the producer said that the five-second interval was strictly kept. When the cards had been run through, Dr Carthy explained that, before he went behind the screen Mr Fisk had had no knowledge of what order the cards that he was to look at would be in.

Behind the screen, G. W. Fisk first opened the paper bearing the target series in the form of a typewritten column of initial letters of the names of the three pictures. Keeping in time with the voice, he ran down the column with his eye, using a stencil to blot out from view the preceding and succeeding letters. As he looked at a letter, so he tried to visualize the corresponding picture card. As soon as the test was over he folded up the paper bearing the targets and later returned it to D. J. West, who kept the paper in his possession and revealed its contents to nobody until some days later, after all the entries had been received, when he posted it to D. Michie for the checking of the results. Thus nobody except G. W. Fisk, D. J. West and perhaps the assistant with the headphones were in the secret of the target order.¹

G. W. Fisk reported that on the first card the voice called out 'one' before he had opened out the paper and was ready to begin. Since the first target was in any case a dummy and not utilized in the assessment, this was not an important point.

EVALUATION OF THE RESULTS OF THE MASS TEST

The experiment was designed to test two distinct alternatives to the null hypothesis: (1) that a large proportion of people would

¹ It is very doubtful, however, whether this assistant could have seen the targets because he stepped back once the headphones were in place while G. W. Fisk was bent forwards over a table with the target paper in front of him.

show weak ESP and (2) that a small proportion would show strong ESP. We shall first consider alternative (1).

(1) *Examination for a generalized effect*

In an experiment where each of a large number of subjects is required to guess a series of cards drawn at random from an effectively infinite pack, each subject being tested with a different and independent series, standard statistical methods are available. We can classify each guess made by each subject as 'right' or 'wrong', pool all the guesses and calculate the standard error of the deviation of the number of right guesses from chance expectation by the use of the binomial formula $\sigma^2 = pqn$. When the subjects are all guessing at the *same* series of targets the use of such a test would require that, in the absence of ESP, the various series of guesses obtained from the different subjects be mutually independent. As has been explained above, this is an assumption which cannot be made in practice; the existence of similar position preferences and sequence preferences among the different subjects is sufficient to cause correlations between their different series of guesses. It was therefore necessary to devise a method of analysing the results of the mass telepathy test which should not require the assumption of independence.

The essence of the method adopted is to split up into two compartments the observed variation in the frequencies with which the different suits in the target pack are guessed. In one compartment any significant variation in excess of the basal (binomial) level can only be attributable to position preferences. In the other compartment both position preferences and ESP can contribute to the excess. The detection of ESP then consists in comparing the magnitude of the second with that of the first department. If it is significantly greater, there is evidence of ESP.

An example of exaggerated simplicity may make clear the principle involved. We consider a pack containing only two suits, e.g. the infinite pack generated by the repeated tossing of a coin. Let us suppose that in a test consisting of 10 guesses the results obtained from 100 participants are as given in Table 1.

It is obvious from the violent variation in the frequencies with which 'heads' is guessed that the 100 participants are highly correlated, that is, they tend to make the same guess at the same time. Is this because they are all 'telepathizing' the same targets, or is it merely because of common preferences, e.g. for starting a series with 'heads' and then alternating, first in blocks of 1 and then in longer blocks?

TABLE I

TARGETS		GUESSES		
Case A	Case B	Heads	Tails	Total
H	H	73	27	100
H	T	16	84	100
T	H	97	31	100
H	T	42	58	100
T	T	34	66	100
T	T	26	74	100
H	H	63	37	100
T	T	57	43	100
H	H	88	12	100
T	H	64	36	100

We can only answer such a question by seeing how much of the variation is target-dependent and how much of it still persists even between guesses aimed at the same target. In the example given above the answer stands out on inspection of the following tabulations :

CASE A.	Numbers of people guessing 'heads'	
Target is 'heads'	73, 16, 42, 63, 88.	Total 282
Target is 'tails'	97, 34, 26, 57, 64.	Total 278
CASE B.	Numbers of people guessing 'heads'	
Target is 'heads'	73, 97, 63, 88, 64.	Total 385
Target is 'tails'	16, 42, 34, 26, 57.	Total 175

In Case A the variation *within* target types is plainly so large that the variation between them (i.e. 282 versus 278) is of no significance. In case B, however, the difference between the total number guessing heads when the target is 'heads' and the numbers when the target is 'tails' is obviously large compared to the variation within target types, and consequently suggests ESP. In less clear-cut cases the amount of variation falling into the 'within' and 'between' departments must be measured quantitatively and the ratio between the amounts in the two departments must be statistically assessed. Before describing how this was done in the present case we must briefly describe the target series used.

This consisted of one of a number of packs of picture-cards of three suits, 'canoe', 'trumpet' and 'wheelbarrow' (referred to henceforth as C, T, and W). The pack which was used was selected at random from 18 possible alternatives all subject to certain constraints, namely that each suit is represented an equal number of times, that each sequence of two suits is represented an equal number of times, and that the different parts of the series are homogeneous with respect to the frequency of occurrence of the three suits. The series so defined belongs to the class of 'serially balanced sequences' described by Finney & Outhwaite (1956), who have investigated their potential use in the design of bioassay experiments. The adoption of such a design in the present experiment was not prompted by any particular advantage over the use of a random series, beyond simplifying the calculations and ensuring a reasonable balance in the target series.¹

The properties of a serially balanced sequence may be illustrated by the series which was actually used in the experiment, namely :

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
W	W	C	T	T	W	C	C	T	W	W	T	C	C	W	T	T	C	W	W

This consists essentially of 18 targets, since the first and last are 'dummies' which were rejected from the analysis. The viewers were not informed of the special properties of the target series.

The statistical analysis of the results proceeds as follows. Let us identify each target by a subscript : e.g. W_3 denoted that target which is the third appearance of suit W, ignoring dummies (actually target no. 10 in the series given above). We now construct a 3×18 contingency table entering for each target the observed frequencies with which the three suits were guessed. This is shown in Table 2, together with marginal total and subtotals for the three target suits separately. From the 3×18 frequencies we compute χ^2 for 34 degrees of freedom, according to the formula

$$\chi^2 = \sum \frac{(\text{observed} - \text{expected})^2}{\text{expected}}$$

¹ I have recently become persuaded that the adoption of a systematic rather than a random series was mistaken. A significant positive result (which did not occur in this experiment) would have been open to the alternative explanation that British TV audiences have a constitutional tendency to think in Finney-Outhwaite sequences. All objections of this character are met by the use of a random target series in combination with the method of analysis described in this article.

The expected frequency for each call is calculated from the marginal totals, e.g. the expectation for each call in the C column is $7365 \div 18$.

TABLE 2

GUESSES

T a r g e t s		C	T	W	Total
	C ₁	393	515	459	1367
	C ₂	354	529	484	1367
	C ₃	417	490	460	1367
	C ₄	388	503	476	1367
	C ₅	428	473	466	1367
	C ₆	401	505	461	1367
	Total	C	2381	3015	2806
		T	2554	2934	2714
	Total	T	2554	2934	2714
	T ₁	453	463	451	1367
	T ₂	414	503	450	1367
	T ₃	437	468	462	1367
	T ₄	403	513	451	1367
	T ₅	436	487	444	1367
	T ₆	411	500	456	1367
	Total	T	2554	2934	2714
	W ₁	442	386	539	1367
	W ₂	395	500	472	1367
	W ₃	404	497	466	1367
	W ₄	390	486	491	1367
	W ₅	386	517	464	1367
	W ₆	413	495	459	1367
	Total	W	2430	2881	2891
	Grand total		7365	8830	8411
					24606

By the same procedure we calculate a χ^2 for 4 degrees of freedom from the 3×3 table constituted by the three rows of subtotals. We call this the 'between-suits χ^2 ' and assess its significance by comparing it, not with its multinomial expectation, but with the residual 'within target-suits χ^2 ' obtained by subtraction, as in the following partitioning.

TABLE 3

		D.F.	Mean square	Mean square ratio
Between target-suits	15.1622	4	3.7906	1.904 $P \approx 0.1$
Within target-suits	59.7311	30	1.9910	
Total	74.8933	34		

The mean squares are obtained by dividing the χ^2 values by the corresponding number of degrees of freedom, and the significance of the mean square ratio is assessed from a table of the Variance Ratio (e.g. Fisher & Yates Table V) (2).

The 'within suits' χ^2 is thus made to yield an estimate of the degree to which the position-preferences of the subjects *have* affected the distribution of guesses among the targets, and this estimate is used as the standard against which to assess the significance of the variation of guesses between target-suits.

It is worth pointing out that this analysis reveals that position preferences *did* in fact play an important role. The expected value of the 'within suits' χ^2 in the absence of such preferences is equal to its degrees of freedom, in this case 30, whereas the observed value is about twice this—significant at the level, approximately, of $P = 0.0001$.

It will be noticed that the test detects a general influence of targets upon the distribution of guesses, and is indifferent to whether the guesses are 'right' or 'wrong'. This special form of non-independence between guesses and targets can be tested separately, as follows :

TABLE 4

	χ^2	D.F.	M.S.
Deviation of the proportion of right guesses from $\frac{1}{2}$	0.0029	1	0.0029
Residual	74.8904	33	2.2694
Total	74.8933	34	

$P > 0.95$

The analyses in Tables 3 and 4 give no significant support for the hypothesis of ESP.

We may make similar tests for the occurrence of 'pre-cognitive' and 'post-cognitive' ESP (forward and backward 'displacement')

by matching the same guesses against targets 1-18 and 3-20 respectively in the series given above.

The corresponding χ^2 analyses for pre-cognitive and post-cognitive effects are as follows. (Tables 5 & 6.)

TABLE 5. POST-COGNITIVE

	χ^2	D.F.	Mean Square	Mean Square Ratio
Between target-suits	8.2455	4	2.0614	} 0.928 $P \gg 0.2$
Within target-suits	66.6478	30	2.2216	
Total	74.8933	34		

	χ^2	D.F.	Mean Square	Mean Square Ratio
Deviation	7.9122	1	7.9122	} 3.898 $0.1 > P > 0.05$
Residual	66.9811	33	2.0297	
Total	74.8933	34		

Here again there is no significant evidence of an association between targets and guesses.

TABLE 6. PRE-COGNITIVE

	χ^2	D.F.	Mean Square	Mean Square Ratio
Between target-suits	5.5339	4	1.3835	} 0.598 $0.8 > P > 0.2$
Within target-suits	69.3594	30	2.3120	
Total	74.8933	34		

	χ^2	D.F.	Mean Square	Mean Square Ratio
Deviation	1.3842	1	1.3842	} 0.621 $0.8 > P > 0.2$
Residual	73.5091	33	2.2275	
Total	74.8933	34		

The viewers were also asked to record their sex and whether they thought that they had scored well or poorly. The results were broken down into sub-categories according to this information, but no deviations from expectation within the sub-categories, of

differences between them, appeared sufficiently striking to warrant detailed statistical examination.

The results of this experiment were thus negative as far as concerns the first hypothesis, namely that there is a small amount of ESP widely distributed through the population.

(2) *Examination of individual scores.*

We now turn to the second alternative, that the overwhelming majority of people possess no ESP, or do not manifest it under the conditions of such a test, but that there are a few rare individuals with strongly developed extra-sensory powers.

From this point of view the TV test can be regarded as a wide dragnet cast to catch one or two valuable fish.

In order to discover whether there were individual scores lying significantly beyond the bounds of expectation all entries were mechanically sorted and those with the highest scores were picked out. The process was repeated three times, once by direct matching and once each for 'pre-cognitive' and 'post-cognitive' matching of guesses with targets.

The 'pre-cognitive' and 'post-cognitive' sortings gave no results of interest. The direct sorting, however, yielded one entry sent in by a Mr Downey with 15 right guesses out of a possible 19. There were 3 'runners-up' with scores of 13. The possible number is 19, not 18 as in the mass test, since only guess no. 1 was counted as a dummy. In the analysis of the mass experiment, nos. 1 and 20 were rejected as dummies for reasons which do not apply to the analysis of individual scores. But no. 1 may be expected to be peculiarly subject to 'position preference' and it was decided to ignore it in sorting for high scoring individuals. This decision was reinforced by the fact that the test was launched rather abruptly under the actual conditions of the broadcast, and it was feared that many viewers would not have been able to get 'settled down' until after the first target. The expectation that no. 1 would prove to be in a special category was borne out by the fact that the three targets, C, T and W were guessed with frequencies 156, 806 and 402 in contrast with the approximate equality of frequencies prevailing throughout the remainder of the series.

Mr Downey's guesses are set out below alongside the target series.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Target series:	W	W	C	T	T	W	C	C	T	W	W	T	C	C	W	T	T	C	W	W
Downey's series:	T	W	C	T	T	W	C	C	T	W	W	T	T	C	W	W	T	T	C	W

The crude method of estimating the probability of obtaining

through chance alone, 15 or more correct guesses out of 19 is to apply the binomial theorem on the assumption that each guess has an independent probability of $\frac{1}{3}$ of being correct, as follows:

$$P = \sum_{r=15}^{19} \left(\frac{1}{3}\right)^r \left(\frac{2}{3}\right)^{19-r} \frac{19!}{r! (19-r)!} = 0.6 \times 10^{-4}$$

The probability of finding one or more scores as good as this in a sample of 1367 entries is

$$1 - (1 - P)^{1367} = 0.080$$

This method is liable to overestimate the unlikelihood of the event, since the assumption of independence only holds if either the targets or the guesses constitute a random series. In point of fact the target series was a highly systematic series, and the guesses of any one subject are likely to be subject to various restrictions, including the tendency to 'balance up' the guesses so that the suits are represented with approximately equal frequency.

We can use an alternative method of assessing the significance of Mr Downey's score which is unaffected by the 'balancing up' tendency.

We calculate the number of possible series having the same frequency distribution among the three suits as his, namely 5 C's, 7 T's, and 7 W's. This is $\frac{19!}{7! 7! 5!}$. We find by enumeration how many of these contain 15 or more correct items. The answer is 1488.

The ratio of the latter to the former quantity gives the probability of obtaining 15 or more correct guesses by chance when matching a series of the same frequency distribution as Mr Downey's against the target series.

This ratio $= P = 1.5 \times 10^{-4}$. As before we calculate $1 - (1 - P)^{1367}$, which in the present case is equal to 0.185.

The above estimate is probably biased in the opposite direction to the first, since it assumes that the other 1366 viewers 'balanced up' their guesses to the same degree as Mr Downey. In actual fact we might expect the majority to fail to balance their series so well; we have not undertaken the labour of sorting and analysing the results to establish this particular point. It seems fair to conclude that the chance of finding, if chance alone were operating, one or more scores as good as Mr Downey's in an experiment of this kind and size lies somewhere between 8 and 18 per cent.¹

¹ It may, however, be argued that no account is here taken of the 'pre-cognitive' and 'post-cognitive' sorting which was done.

This result does not reach the conventional level of statistical significance, but was judged sufficiently suggestive to prompt further tests of Mr Downey's ability. An additional circumstance which made further work seem worth undertaking was a peculiar feature of this subject's 4 mistakes: they are all correct if viewed as 'post-cognitive' guesses, as can be seen by examination of his series of guesses given above. Looking at it another way, if it is assumed that at some stage,—either at guess 13 or guess 16,—Downey 'fell behind' the series of targets and lagged by 1 interval until the end of his series, then all guesses but one are correct.

Mr Downey was accordingly approached and agreed to be the subject of further tests.

FOLLOW-UP TESTS WITH MR B. DOWNEY

Mr Downey took part in five subsequent experiments which were conducted by G. W. Fisk and D. J. West. After that the investigation was brought to a close because the subject's enthusiasm was waning and the tests were becoming burdensome to him.

The results not only showed evidence of an ESP effect, but were interesting in that there was a marked difference according to whether G. W. Fisk or D. J. West was carrying out the experiment.

Experiment I. This was carried out by Mr G. W. Fisk, who writes as follows:

I met Mr Downey on the morning of April 30th in the Park Street offices of Science Television Services Ltd., and on this occasion I gave him a few more ESP trials. Also present were Mr M. Goldsmith and Mr M. Wiltshire of the Television company, Mr Downey junior and, for the latter part of the session only, Mr Denys Parsons.

I first tested Mr Downey with 4 runs of clock cards (GESp tests) using four sealed packs of 12 which I had previously made up against a series of random numbers. I sat at a table with my back to the percipient and the witnesses. Mr Downey was seated with his back facing me at the opposite corner of the room—a distance of about 15 ft. Conditions were as good as possible under the circumstances and I am reasonably satisfied that visual and auditory clues were excluded. When the test was due to begin I opened the envelope containing the pack, placed it face down, and turned over the cards in succession giving a tap of my pencil for each exposure. Mr Downey wrote down his guesses himself, calling 'right' each time as a signal that he had registered a guess.

None of the witnesses was permitted to look over my shoulder to see the target cards. At the conclusion of the test I collected the record sheet and with the help of Mr Wiltshire I entered the target order beside the subject's guesses. I did not analyse the scores until later, but I could see that Mr Downey had made a considerable number of hits.

Experiment II. On May 3rd 1957 a further run with clock card was carried out with Mr Fisk as agent. The experiment was transmitted by television and viewed by the public, but the results were unspectacular.

Experiment III. On June 2nd and 3rd 1956 D. J. West visited Mr Downey at his home in the Midlands accompanied by an assistant. Friendly relations were established with Mr Downey and his family, and 9 runs of clock-card tests were tried under varying conditions and at different times. The results throughout were so close to chance expectation that the attempts were discontinued.

In this experiment there were 9 runs of clock card trials in all, of which two were runs of clairvoyance trials using randomized cards in closed envelopes whose contents were unknown to anyone present. There were 6 telepathy runs with the subject in a separate room from the agent, but within earshot. In five of these D. J. West acted as agent while his assistant supervised the subject. In the remaining run, D. J. West, his assistant, and Mr Downey's younger son all looked at the target cards. The remaining run was performed with D. J. West acting as agent and Mr Downey in the same room.

Experiment IV. From June 11th to July 7th 1956, Mr Downey took part in a series of clock-card trials conducted at a distance. As usual the target cards were in random order. One card per day was put out as target. It was set up every week at approximately 9.0 A.M., either by D. J. West in his room at Hampstead, London, or by G. W. Fisk at Ditton Hill, Surrey. Mr Downey, who remained at his home in the Midlands, about 100 miles away, was asked to record his impression at any time of day he felt like doing so. D. J. West and G. W. Fisk acted as experimenter on alternate weeks, and on each occasion Mr Downey was informed who was the experimenter.

Experiment V. This was a distance test similar in all respects to the previous one except that a sequence of three cards was put out each morning and Mr Downey was required to make three guesses per day.

It will be noted that there were witnesses present at the first three experiments, but that the precautions against sensory leak-

age, although probably adequate for practical purposes, did not formally exclude all possibility of the unwitting transmission of auditory cues. The last two experiments were distance tests in which sensory leakage was automatically excluded. In these two series the experimenters worked alone without witnesses, but the targets were always recorded before receipt of Mr Downey's guesses so as to eliminate recording errors.¹

For the sake of completeness it should be mentioned that on the occasion of the first experiment, on 30 April 1956, two runs with the conventional five-symbol ESP cards were carried out after the clock-card test. He scored six hits in 50 trials, 4 less than chance expectation. These have not been included in the statistical evaluation. All the other trials were with clock-cards as targets, the subject being required to guess at which of the twelve hour positions the finger depicted on the card was pointing. A full description of the use of clock cards and the divergence system of scoring—which gives proportionate weight to near misses—has already been given in this *Journal* and need not now be repeated (4).

RESULTS OF THE FOLLOW-UP TESTS

The scores obtained by Mr Downey in all the clock-card tests are set out in Table 7.

In the analysis of these results we shall consider only the divergence scores since they contain more statistical information than the direct hit scores.

For any run of n guesses the chance expectation for the divergence score is $3n$. The amount by which the observed score falls short of chance expectation (i.e. $3n$ minus the divergence score) is called the divergence score deviation. Positive divergence score deviations would be produced by above chance ESP scoring. An indication of the rate of scoring is given by the mean divergence score deviation, which is simply the divergence score deviation divided by the number of trials, n . The chance expectation of this value is zero.

As shown in Table 7, we have the results of 30 runs, from each of which a mean divergence score deviation can be calculated. This has been done in Table 8 for 28 out of the total 30 runs. Runs 6 and 7 were discarded from the evaluation because they were conducted under 'clairvoyance' and not under 'telepathy' conditions, and hence have no agent. As will be seen from the analysis given later, the identity of the agent proved an all-important factor in Mr Downey's performance, and in any case it was on the basis of a

TABLE 7

Experi- ment No.	Run No.	No. of trials in run	Direct hit score	Diver- gence Score	Date	Agent	Type of Experiment
I	1	12	2	26	30/4/56	Fisk	Telepathy, same room
	2*	11	1	31			
	3	12	2	24			
	4	12	4	20			
II	5	12	2	34	3/5/56	Fisk	Telepathy televised.
III	6	12	0	38	2/6/56	—	Clairvoyance. Closed en- velopes.
	7	12	1	25		—	
	8	12	1	38		West	Telepathy, separate rooms.
	9	12	2	28		West	
	10	12	1	40		West	
	11	12	0	49		West	
	12	12	0	38		West	Telepathy, same room.
	13	12	1	37		West <i>et al.</i>	
	14	12	2	39		West	
IV	15	6	0	18	11-16/6/56	West	Distance tele- pathy. One guess per day.
	16	6	1	12	18-23/6/56	Fisk	
	17	6	2	12	25-30/6/56	West	
	18	6	2	13	2-7/7/56	Fisk	
V	19	6	2	14	16-17/7/56	Fisk	Distance tele- pathy. Three guesses per day.
	20	6	1	16	18-19/7/56	Fisk	
	21	6	0	12	20-21/7/56	Fisk	
	22	6	0	22	23-24/7/56	West	
	23	6	1	16	25-26/7/56	West	
	24	6	2	15	27-28/7/56	West	
	25	6	1	16	30-31/7/56	Fisk	
	26	6	2	9	1-2/8/56	Fisk	
	27	6	0	13	3-4/8/56	Fisk	
	28	6	0	23	6-7/8/56	West	
	29	6	0	19	8-9/8/56	West	
	30	6	1	14	10-11/8/56	West	

* There was one call missing in run 2. Chance expectation for the divergence scores is $3n$, where n is the number of trials. A low divergence score indicates a positive deviation.

telepathy test that Mr Downey was initially selected for further study.

The statistical problem is to decide whether the 28 mean divergence score deviations of Table 8 differ in aggregate from zero to a significant degree, or whether they show other regular departures from chance expectation. The method adopted is essentially that of a weighted analysis of variance. Since not all the runs are composed of the same number of trials (n), it is clear that they

should be given different weights, proportional to the value of n . A simplification can be introduced by taking advantage of the fact that the mean divergence score deviation has a known theoretical variance of $19/6n$. By taking the reciprocal of this quantity as the weight for each run, and computing the weighted sums of squares, we arrive at a set of values which are distributed as χ^2 and can be evaluated directly from a χ^2 table. The full analysis is set out in Table 8 and summarized in Tables 9 and 10.

TABLE 8

Experiment No.	Agent	Run No.	n.	Divergence Score Deviation	Mean Divergence Score Deviation (\bar{x})	
I	Fisk	1	12	10	0.8333	
	"	2	11	5	0.4545	
	"	3	12	12	1.0000	
	"	4	12	16	1.3333	
II	"	5	12	2	0.1667	
IV	"	16	6	6	1.0000	
	"	18	6	5	0.8333	
V	"	19	6	4	0.6667	
	"	20	6	2	0.3333	
	"	21	6	6	1.0000	
	"	25	6	2	0.3333	
	"	26	6	9	1.5000	
	"	27	6	5	0.8333	
Weighted mean of \bar{x} = $\Sigma w\bar{x}/\Sigma w = 0.7850$						
					χ^2	D.F.
FISK	Departure of mean of \bar{x} from zero :				$(\Sigma w\bar{x})^2/\Sigma w = 20.8235$	1
	Heterogeneity :				$\Sigma w\bar{x}^2 - (\Sigma w\bar{x})^2/\Sigma w = 5.1045$	12
	Total				$\Sigma w\bar{x}^2 = 25.9280$	13
III	West	8	12	-2	-0.1667	
	"	9	12	8	0.6667	
	"	10	12	-4	-0.3333	
	"	11	12	-13	-1.0833	
	"	12	12	-2	-0.1667	
	"	13	12	-1	-0.0833	
IV	"	14	12	-3	-0.2500	
	"	15	6	0	0.0000	
V	"	17	6	6	1.0000	
	"	22	6	-4	-0.6667	
V	"	23	6	2	0.3333	
	"	24	6	3	0.5000	
	"	28	6	-5	-0.8333	
	"	29	6	-1	-0.1667	
	"	30	6	4	0.6667	
Weighted mean of \bar{x} = $\Sigma w\bar{x}/\Sigma w = -0.1000$						
					χ^2	D.F.
WEST	Departure of mean of \bar{x} from zero :				$(\Sigma w\bar{x})^2/\Sigma w = 0.3789$	1
	Heterogeneity :				$\Sigma w\bar{x}^2 - (\Sigma w\bar{x})^2/\Sigma w = 12.2790$	14
	Total :				12.6579	15

The weight w for each \bar{x} is $6n/19$.

TABLE 9

	χ^2	D.F.	P
Overall Deviation	7.2113	1	<0.01
West versus Fisk	13.9910	1	<0.001
Residual heterogeneity	17.3835	26	>0.8
Total	38.5858	28	

TABLE 10

	Mean	χ^2	D.F.	P
Fisk Deviation	0.785	20.8235	1	<0.00001
West Deviation	-0.100	0.3789	1	insignificant

Table 9 shows that there is a significant overall excess of the 28 means over zero ($\chi^2_{(1)}=7.2113$, $P<0.01$). In addition there is a highly significant discrepancy between the results obtained by the two agents ($\chi^2_{(1)}=13.9910$, $P<0.001$). This latter effect is independent evidence against the null hypothesis. The combined significance is measured by $\chi^2_{(2)}=7.2113+13.9910$, $P<0.00001$.

When the results of the two agents are set out separately in Table 10 it can be seen that the entire deviation above chance is concentrated in Fisk's results, for West actually achieved a small negative mean score. The statistical significance of Fisk's data taken alone is very high indeed ($\chi^2_{(1)}=20.8235$, $P<0.00001$).

Although the difference between Fisk's and West's results was unexpected and unpredicted, the observation fits in with some previous work in which these two experimenters both tested the same set of subjects. It was found then that the general run of subjects scored significantly with Fisk but not with West (5).

Finally we may re-test Fisk's data after rejecting all but the 'distance telepathy' series. This provides a most stringent test, since all possibility of sensory leakage is excluded.

The appropriate statistics are given in Table 11.

TABLE 11

Mean of $x = \Sigma wx / \Sigma w = 0.8125$

	χ^2	D.F.	P
Departure of mean of x from zero :	10.0066	1	<0.01
Heterogeneity :	1.9406	7	
	11.9472	8	

The results still attain a high level of statistical significance, even after almost half the data have been discarded. Comparison with Table 8 shows that the rate of scoring is approximately the same in the distance tests as in the remainder; the actual mean divergence score deviations are, respectively, 0.8125 and 0.7627.

DISCUSSION

Perhaps the greatest obstacle to progress in the field of ESP research has been the elusiveness of the phenomena under investigation. In consequence although a great deal of work has been devoted to seeking evidence of such phenomena, almost nothing has been learnt about their causal basis.

In our opinion nothing is likely to be learnt until means are found of eliciting ESP phenomena in a strongly-manifesting and readily repeatable form. This was the position with respect to electricity, so long as men's experience of electrical phenomena was limited to thunderstorms and the occasional spontaneous occurrence of electrostatic effects.

In the case of ESP there are two ways of attempting to enhance the phenomenon to a level at which detailed causal analysis can be undertaken more readily. The first is to increase scoring capacity by transforming the mental state of the percipient and for agent, using drugs, hypnosis, etc. So far this approach has had little spectacular success. The second approach is to screen a large number of percipients in order to discover one or more with a rare capacity for consistent high scoring. The present work comes under this second heading.

In order to judge whether our approach is fruitful, we need to evaluate the data of the follow-up tests of the Fisk-Downey combination (the West-Downey series is not relevant to the present topic, since Fisk, not West acted as agent in the mass test by which Downey was selected) relative to the scoring rates achieved by other subjects under other conditions.

A simple way of doing this is to calculate the rate of scoring which would be needed using the more conventional target systems in order to achieve the same level of statistical significance with the same number of guesses. The Fisk-Downey series (see Table 8) gave a $\chi^2_{(1)}$ of 20.8235 in 107 guesses, that is, a mean contribution to $\chi^2_{(1)}$ of $\frac{20.8235}{107} = 0.1946$ per guess.

The rates of scoring with Zener cards and dice which could produce the same statistical significance are as follows :

TABLE 12

Target System	Chance Expectation of per cent hits	Per cent hits to produce Fisk-Downey level
Zener type ESP cards	20%	38%
Dice	17%	33%

Thus, to produce the Fisk-Downey level of significance would require an average of 9.5 hits per run of 25 conventional ESP cards, an average that has been achieved by very few subjects.

SUMMARY AND CONCLUSION

A mass telepathy test was conducted over television, in which G. W. Fisk acted as agent and the audience were subjects. No evidence of a generalized ESP effect was obtained, but one entrant, Mr B. Downey, produced a score which was considered sufficiently suggestive to warrant further investigation.

A series of tests with Mr Downey gave satisfactory statistical evidence of an ESP effect in telepathy trials using clock cards. In addition, analysis showed a highly significant difference between Mr Downey's results with the two experimenters West and Fisk. He was completely unsuccessful with the former.

This experiment shows the practicability of using television for a mass test and it illustrates the use of a relatively simple method of dealing with the difficulty of sequence and position preferences. But the results give no encouragement to the hope that unstructured tests with large unselected audiences will yield generalized positive scores. On the other hand, this instance of the successful isolation and further testing of a subject with marked ESP ability shows that mass tests can be used for discovering good subjects. The unexpected features of Mr Downey's subsequent results illustrate once again the importance of the experimenter's role in the genesis of ESP scores.

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ADDENDUM

The Fisk-Downey series can be made to yield an answer to an important question. Was the high score achieved solely through the above-chance frequency of 'direct hits' on the target, or was a significant contribution also made by a non-random distribution of the 'misses'?

After subtracting the 20 'direct hits' from the total of 107 guesses, the remaining 87 are set out below for comparison with the chance expectations.

Divergence of :	1	2	3	4	5	6	Total
Observed							
frequencies :	26	15	20	12	11	3	87
Chance							
expectation :	15.82	15.82	15.82	15.82	15.82	7.91	87.01
Observed, as							
per cent of							
expected :	164	95	126	76	70	38	

The downward trend of the percentages in the bottom row is unmistakeable ($P < 0.02$). The Fisk-Downey result cannot be fully accounted for by the high number of 'direct hits'.

THE LOGICAL AND SCIENTIFIC IMPLICATIONS OF PRECOGNITION, ASSUMING THIS TO BE ESTABLISHED STATISTICALLY FROM THE WORK OF CARD-GUESSING SUBJECTS

BY L. C. ROBERTSON

BEFORE proceeding to the implications in the assumption that precognition has been established statistically from the work of card-guessing subjects, it may be remarked that in this context the use of the term *guessing* as a correlate of precognition is unfortunate, for it seems to slur over the psychological distinction between mere guessing and actual foreknowledge. However, this point need not be pressed since the assumption we are asked to start with is the proved possibility of the precognition of the order in which sequences of cards will be drawn, the precognizer not being in a position to manipulate the cards in any way, and not having any data of any sort from which rational inferences pointing to the results of the draws could be made. If the truth of such experimental paranormal phenomena be accepted what are its logical or scientific implications? Perhaps none of any relevance to the present discussion, for it may quite simply be a brute empirical fact that paranormal precognition is confined to the restricted field of card sequences. But this would surely be to adopt too uncompromisingly rigid an attitude. Logically we are entitled to take a further step. In the absence of facts pointing to the necessary restriction of such phenomena to so narrow a field it is legitimate from the card-guessing experiments to infer the possibility of precognition of future events in general. This extension of the field is justified too by the enormous mass of data furnished by the records of well-authenticated cases of precognitive phenomena in ordinary life. In fact, as we know, it is this impressive recorded history of such spontaneous cases which led to the 'card-guessing' experiments specially devised in order to bring precognitive phenomena under scientific control.

Accepting, then, the evidence for paranormal precognitive phenomena without restriction to any particular field of events, what follows? It is here that some, even among those professionally engaged in the study of para-psychology, are apt to jump to hasty conclusions. Professor Rhine, for instance, in *The Reach of the Mind*, says with a touch of emotionalism unusual in scientific writings, that 'if precognition is or could be 100 per cent accurate, the knowledge of that fact would so profoundly affect our philo-

sophy of life that one shudders at the implications'. He is referring here to the popular notion that absolute precognizability implies a completely determined order of events, leaving no room for freedom of choice. We have only to call to mind Origen's famous saying : 'God's prescience is not the cause of things future, but their being future is the cause of God's prescience that they will be,' to realize that it is not a matter of simple logic that complete precognizability implies a mechanistically determined closed system fatal to all freedom of choice. There can be no doubt, however, that with the acceptance of the evidence for precognition must come a considerable reconstruction of scientific cosmology. The phenomena of precognition undoubtedly clash with certain 'basic limiting principles of ordinary thought' as Professor Broad calls them. The chief of these, perhaps, is the principle of the priority in time of the cause to its effect. The most important implications of paranormal precognition are those bearing on the notions of freewill (a misleading term), causality, and time. These implications are, of course, not logically independent of each other, for both the freewill and the causal problems, even apart from the paradoxes introduced by precognition, are intimately connected with the basic question of time and temporal perception, and cannot be discussed without reference to it.

It is only briefly that I propose to touch on the bearing of precognition on freewill, and in doing so I shall by-pass all discussion of the highly controversial problem of determinism and indeterminism, and quite simply assume the fact of freedom of choice. It is often argued that if one could precognize a certain disastrous event in one's life, then one could, assuming volitional freedom, prevent it from happening. The obvious fallacy here, of course, is that if one could intervene to prevent the occurrence of the disaster, and actually succeeded in doing so, that disastrous event would not be an event in one's life, and therefore the so-called precognition of it not a precognition at all. This raises the question whether there could be a true cognition by anyone of a future disastrous even in his own life. Assuming the person concerned to be normally constituted in that he would naturally seek to avoid disasters of any kind, and assuming too that he believed in the truth of the precognition, the answer is that he could have such a precognition if the disastrous occurrence were to take place in spite of precautions taken but occurred in unforeseeable circumstances, that is, in an unprecognized way. In other words, there could not be a full precognition by anyone of all the circumstances leading up to a disastrous occurrence in his life, including the precautions taken by him to avoid it. It does not follow from this

that the really free acts in anyone's life cannot be precognized by him, for there is no apparent reason why a person should not be able to precognize future events in his life of such a nature that the precognition of them would not cause him to try to prevent them from happening, even by way of an experiment to ascertain whether he could intervene. And, of course, there is nothing illogical in A being able to precognize future free acts of B, provided he could not interfere with them or cause B to do so. Though not to be inferred from any of the card-guessing experiments, it would not be out of place to mention here certain quasi-precognitive phenomena, for precognition is not restricted to future events that actually occur precisely as precognized, but also to events that are subsequently recognized as having been definitely possible in certain situations the main features of which are veridically precognized. I refer here to paranormally foreseen potentialities of disasters and to premonitions in general, and I speak of them as quasi-precognitive since they are concerned in part with future events which did not occur though they could have occurred in the circumstances so far as they were precognitively revealed.

Since in paranormal precognition we are confronted with the seeming paradox of effect preceding cause, it is obvious that drastic changes are called for in the current scientific notion of causation, in which lies a deep-rooted prejudice in favour of the belief in the cause necessarily preceding what we regard as its effect. Precognitive phenomena clearly imply some mode of causation independent of the time-order, or possibly, some revolutionary alternative principle to that of causality awaiting discovery and formulation by some genius of the calibre of a Newton or an Einstein.

One of the difficulties raised in the popular mind by non-inferential precognition is that it seems to involve a belief in the direct awareness of what, being still in the future, is regarded as non-existent. How can one perceive what does not exist? This is the form taken by the objection. But the difficulty is due to an obvious misconception, and vanishes with the realization of the fact that what is immediately prehended is not the future events themselves. In precognition the present perception or awareness is merely of contemporary images referable in thought to the future. This explanation, however, leaves untouched the crucial point that the real causes of such contemporarily prehended images and of the beliefs based on them are in the future, and therefore, no more existent *now* than are events in the past which are merely remembered. Attempts to get over this impasse serve to bring to

light the deeper implications of the precognitive situation. The evidence for paranormal precognition necessitates a revision of the ordinary view of temporal perception and that of time itself. But we are not in possession of sufficient data, and lack the necessary insight to frame a new concept of time in the light of which the startling facts of paranormal precognition may be satisfactorily explained. Several tentative hypotheses have been put forward, the essential feature in all being the abandonment of the notion of a one-dimensional time. Professor C. D. Broad has, with his usual logical persuasiveness, set forth a theory in which in order to explain precognition he ascribes a second dimension to time. Professor H. H. Price, no less acute a thinker, though at many points critical of Professor Broad's theory, does not reject it as untenable. He, however, prefers the explanation of precognition to be given in terms of telepathy and not in terms of memory traces. Since it falls outside the scope of this short essay to enter into the *modus operandi* of precognition, its object being merely to indicate the implications emerging from its acceptance as a fact, there is no need to discuss the merits of the arguments employed by Professors Price and Broad. Nor is it necessary to make more than a passing reference to Mr Dunne's well-known theory of Serialism, framed by him for the explanation of veridical foreseeing. Dunne, as we know, 'spatialized' time as a moving line, and in order to be able to view the motion of his linear field objectively, was driven to the postulate of a second time dimension, and so to an infinite regress of times. His theory, which with modifications suggested by some of his critics, among the ablest of whom is Professor Broad, must be admitted to be a plausible one. But it should be remarked here that in our present imperfect state of knowledge no single one of these theories holds the field to the exclusion of the others. Yet, though in regard to the implications of precognition much is still conjectural, the conjectures, being logically based, as they are, on factual premises, are not of the sort to be dismissed as belonging to the world of fantasy.

The central difficulty in any study of the nature of precognition is in regard to the status of the future. Considerations already touched on above, compel us logically to the view that what appears to be future is not really future in the sense of being non-existent, and we cannot but hold that there is something delusive in ordinary perception with its successive phases of past, present and future. This leads us to another implication of precognition, namely, that whatever its uses may be as a mathematical conception, time is not in reality a unilinear, irreversible series of events flowing in one direction only from the past to the future, but must be regarded

rather as a *totum simul* in which past, present and future co-exist, though they appear to normal consciousness to be successive phases in a temporal flux. In the psychological phenomenon of 'the specious present' we have actual experiential evidence to rebut any charge of nonsense that may be brought against the notion.

In the specious present or the time-span of ordinary consciousness we have, as it were, a duration-block in which indubitably successive events with distinctions of before-and-after are also manifestly contemporary in time. Its reality cannot be questioned as it is not a matter of inference but of direct experience open to introspection. Its temporal extensity is estimated to range from about half a second to as much as four seconds. It varies with different individuals and in different psycho-physiological and other conditions such, for instance, as those induced by certain drugs. This variability of the time-span of consciousness together with its unique feature of being an enduring present in which past, present and future co-exist, albeit only momentarily, carries with it an implication of profound significance to the whole question of precognition, for it means that A may now be perceiving an event which for B is already only a memory, and similarly an event still in the future for X may for Y be a present percept or a remembered event. Bertrand Russell in his *Human Knowledge : Its Scope and Limits*, speaks of 'a complete complex of compresence' in reference to the past, present and future being compresent together in the one unitary moment of experience. Precognitive phenomena certainly suggest an extension of compresence beyond normal limits.

Admitting a specious present in which the elements of pastness and futurity are not delusive, though some philosophers, S. Alexander, for one, in his *Space, Time and Deity*, have argued to the contrary, the possibility of prehending future events may be plausibly accounted for by the assumption of an abnormal time-span of consciousness extending to wider fields of compresence, and embracing in the immediacy of its prehension events which are past and future for the normal specious present. Such an explanation of the precognition of distant future events by supposing an extension of the specious present beyond its normal limits is not novel. F. W. Myers, that pioneer psychical researcher, or parapsychologist as he would now be called, suggested it more than sixty years ago. Knowing what we now do about the subconscious, which seems to be the source to which most precognitive phenomena may be traced, it seems reasonable to assume that the specious present of the subconscious or 'subliminal' self, or the paraperpersonality, or of what William James terms the 'B' region of

consciousness of any individual, is co-terminous with the whole of his life. This provides an explanation of the precognition by any individual of the future events of his own life. Going a step further, it would not be far-fetched, illogical, or unscientific to invoke the explanatory aid of telepathy between individuals to account for paranormal precognition by a percipient of past or future events in the lives of others.

THE AVAILABILITY OF PSI FOR RESEARCH

A RESEARCH PROGRAMME FOR STUDYING THE NEXT SUCCESSFUL CARD-GUESSING SUBJECT

BY P. R. F. CLARKE¹

THE point of investigating natural phenomena is to explain them. Much has been written of what constitutes a scientific explanation and I do not propose to discuss it here. Whatever the ultimate explanation demanded, the activity of scientists appears to involve establishing generalizations which provide material for the construction of theories. These in turn provide predictions to be verified by further experimentation, and thus gradually work into a more and more coherent body of knowledge. Appropriate programmes for research naturally depend on the state of the development of the particular discipline. In designing experiments in parapsychology we do not have any theories exact enough to provide much direction to our research: we are still faced with the problem of establishing initial generalizations. Fortunately parapsychology can turn to the slightly more coherent body of psychological knowledge for experimental inspiration.

The problem of obtaining a reliable source of psi ability with which to experiment is perhaps the one which parapsychologists would most like solved. This is not the problem of experimental control of the ability—we cannot hope to be able to manipulate psi processes accurately until we know a great deal more about them—this is merely the problem of availability of experimental raw material. The answer to this problem might of course also greatly help our fundamental research into the nature of psi. There are at least three ways in general that the raw material could be made more readily available: first, by a refined measuring technique

¹ I should like to acknowledge here the generosity of A. R. Jonckheere of University College, London, in giving me statistical advice and general criticism.

which could pick up slighter effects ; second, by improving the subject's conscious control of the ability, and third, by better control of the experimental situation. Something has already been done on all three aspects of this problem. In outlining a research programme for use with the next good card-guessing subject, I propose to turn to the techniques and generalizations of the psychologist to improve our experimental supply of psi. Since our subject is good at card-guessing, his known ESP ability should be studied rather than putative PK ability. The method—GESP or clairvoyance—with which he is most successful should be used at first, though later replication with the other 'modality' would be valuable.

The use of clock cards (12) was one attempt to provide a more sensitive instrument by scoring 'near misses'. I propose to present one set of stimuli repeatedly in the hope that the subject will gradually learn them and so improve his score, just as a subject repeatedly presented with visual material too brief to be perceived at first will gradually piece it together accurately. If this can be done, it should provide a lens by which the most ordinary psi ability could be magnified to experimentally useful proportions. It needs to be done initially with a good subject so that a negative result would have some significance as well as a positive result. A positive result is at least plausible. Learning occurs through normal channels of information with repetition of material too complex or brief for immediate registration. Moreover Pratt (7), among others, has drawn attention to the similarity between learning curves and ESP salience curves.

As well as the immediate usefulness of a positive finding it would be particularly stimulating to research to bring together the topics of psi and learning because learning has been studied extensively and relatively successfully in psychology. A fair number of generalizations have been established and some exact theories erected to provide predictions for experimental confirmation. Should a positive result be obtained it would be valuable to investigate the variables concerned and my proposed programme would leave room for this.

The second way I suggested of improving the availability of experimental material is to increase the subject's conscious control of his ability. A number of people have suggested doing this by delving analytically into the unconscious and releasing inhibitions or studying the social relationships involved. McElroy and Brown (4) attempted to teach the subject by shocking him when he guessed wrong. This certainly appeared to have an effect on motivation but no learning trend was observed. This may have

been due in part to the design of the experiment or the lack of appropriate statistical measures. Certainly this 'training' approach has not been fully explored and it would be useful to investigate it further in this project.

My third proposal for improving the availability of psi for research—the better control of the experimental environment—is less exact. Much experimental effort has been expended already on the relationships between personality, personal values and psi ability, largely from the nomothetic approach. In the case of a high scoring subject the idiographic approach would appear of value. Unfortunately this work must suffer from the controversial status of personality measures in the field of psychology. So far we generally have only 'clinical' impressions of what environmental factors favour psi to guide us. I suggest that a thorough assessment of the subject, experimenter, and any agents used with a variety of personality and aptitude tests might provide clues for manipulating the experimental situation. It would probably also be useful to have measures of individual differences in a number of phenomena of the psychological laboratory. It would be interesting to study the physiology of the subject when exercising psi (Otani (5) has already found some relationships between good scoring periods and the psychogalvanic reflex). This might be done by recording such factors as the skin resistance, pulse and respiration rate during the experimental session. A study of the temporal relationships between presentation of the stimulus and the response might be revealing too. Perhaps most important of all would be the experimental manipulation of motivational variables. If positive results with learning had been obtained and the method proved useful experimentally, the relative value of massed and spaced trials should be determined.

All these suggestions sound a tall order for one hundred hours' research. However, by appropriate experimental design and instrumentation many of these suggestions can be tested at the same time. I shall outline a possible experimental method without much detail and suggest briefly some apparatus which could be used for these experiments and which has, I think, some particular merits.

The initial experiment should be devoted to the problem of whether or not the subject can improve his score on a series if it is presented several times. One of the virtues of this method is that we need not concern ourselves with the problem of chance in assessing the effect of psi: we need only establish that an upward trend exists and infer from our experimental control of all normal sensory channels that it is due to psi.* Our subject should be

tested with a number of such series, each being presented perhaps ten or more times. To make the task appear reasonably easy to the subject the series should be fairly short, say ten to fifteen calls and perhaps only two choices at each call. This would be analogous to learning a maze with ten two-way choice-points. It would be aesthetically and economically pleasing if we could use a factorial design for our experiment to study the learning under varying conditions, but at this stage there are grounds for demanding less from our data. The statistic which would be most appropriate for assessing the presence of learning is similar to Kendall's τ . It is S , a function which Jonckheere (1) has developed to detect the presence of an upward or downward trend in successive rankings. I will relegate the details of its calculation to an appendix. It is enough here to say that we can calculate it for our data by regarding the number of hits in each presentation of the series as a rank (0 to k where k is the number of calls in the series). While this statistic has the merit that it makes no assumptions about the distribution of the data, it cannot unfortunately be compared with values of S obtained in other blocks of series. Because of this, comparison between varying conditions would have to be carried out in some other way. It might be possible to use the total number of hits per block as the score with which to do an analysis of variance but that would require the data to be normally distributed and to have roughly equal variances: requirements which would be very difficult to meet with certainty in such small samples as would be practicable. These statistical considerations and the possibility that the subject might show overall practise effects would make a factorial design difficult to handle at this preliminary stage. To assess the presence of learning, then, we could calculate S and divide it by its standard deviation to get a critical ratio. This would be done for each block separately; to get an overall picture we could sum the S 's and then their variances and divide as before.† By calculating a single S and Variance S for all the blocks together we could assess any consistent upward or downward trend overall which would reveal either decline or practise effects. That is, it would help to answer our second question whether the subject can learn to learn. We should of course also be interested to see if the salience effect usual in both learning and psi experiments was present, i.e. if the first and last sections of the series were learnt more rapidly than the central section. This could be assessed by a t-test or by a chi squared. It would also be important to check whether the subject's guesses were becoming more stereotyped over the blocks of repetitions. It is quite possible that they would, and this could again be assessed by the S method, this

time regarding the similarity to the final guess-series as the criterion of ranking. It would be interesting if there were differences in degree of stereotypy between blocks showing good learning, and those showing poor learning. In this preliminary experiment it would be wisest to follow the clinical suggestions which have been made about favourable conditions for ESP for instance by Rhine (8) and also design our experiment to favour the learning. In other words the series should be kept reasonably short, they should be presented fairly rapidly so that they can be perceived as a whole, and there should be definite breaks between runs through the series. The subject should be allowed to feel at ease, and the task should have some 'meaning' for him. These conditions appear to be favourable in general to both learning and psi experiments.

The second question to be answered is, can the subject learn to learn? As I have already suggested, some practice effects may have already appeared in the first experiment, and this might lead us to modify the second: it should however prove valuable whatever the result of the first. Essentially, it is merely a matter of presenting the subject with a series of stimuli obtained according to the most convenient randomizing technique, giving him knowledge of results in some way to enable him to identify any 'feelings' which consistently occur with his correct guesses, and assessing his scores for improvement. McElroy and Brown (4) used an electric shock as punishment for wrong guesses. This appears rather a forceful method, though their subjects showed most of their positive results during shocked trials. Probably a simple knowledge of results situation would be sufficiently rewarding. If it could be arranged, a conditioning set-up might prove more useful in view of the involuntary or unconscious nature of the ability. Hudgins (9) carried out experiments in which the involuntary pupillary reflex to a light was brought under conscious control by pairing a light with another signal plus the subject's saying 'Contract!' and then gradually dropping out the intermediary conditioned stimulus. This is however not directly applicable to our situation because the response has to precede the signal and 'backward' conditioning is a doubtful phenomenon. At best our subject could be conditioned to assess his correct guesses as correct. For us to use a conditioning model we have to assume some 'drive to guess right' which is naturally rewarded by a signal that the guess is right. In our situation the postulated 'feelings' accompanying correct guesses would be the stimulus to be associated with the 'hit' signal (the unconditioned stimulus eliciting the judgment response). While this set-up is rather far removed

from the classical conditioning situations, the extrapolation is perhaps just legitimate. It would probably be valuable to design the experiment to allow both 'conditioning' and 'learning' to operate. Thus as well as giving the subject a signal when he guessed right, he should be told to press a key marked 'correct' and say to himself 'correct' whenever the signal is given. This should allow both processes to occur. The assessment of an improvement in scoring or judging would be more difficult. We could obtain an overall picture by means of the *S* statistic, but over a series long enough to show much 'transfer' effect, decline effects might well confuse the issue. Or we could test the subject on a number of ordinary trials, subject him to the training method, and then give him a test series of ordinary trials which could be compared with the first series in the usual way for differences in extrachance scoring. A similar method is customarily employed in testing conditioning: a number of trials are included in which the conditioned stimulus is presented but not the unconditioned stimulus. Thus we would assume that the 'feelings' occur, but we would not present the 'hit' signal still expecting the subject to make his judgment correctly if conditioning has occurred. These test runs should be interspersed at wide intervals in the training period. Conditioning could be said to have occurred if there were an upward trend in correct judgments over the series of test runs (using the *S* statistic again).

The third mode of attack on the problem of improving the experimental supply of psi is less definite and more exploratory. It is an attempt to discover relationships between the environment, the individual and psi functioning so that it may be more adequately tapped. The first step towards controlling the social situation would be to find out more about the individuals concerned. Thorough assessment with tests of aptitudes and intelligence such as the Differential Aptitude Test, the Wechsler and the Nufferno Speed and Level Tests would seem a useful beginning. Attitude, interest and personality tests would also be advisable: the choice of particular instruments would depend on one's psychological orthodoxy. Measures of reaction time, of oscillation rate observing an ambiguous figure, of figural after-effect displacement, and of perseveration might offer clues as well, since there is some suggestion that individual differences in these respects are related to personality characteristics. Measures of conditionability and learning might also be valuable as a baseline for some of the other data. These tests should be interspersed with the ESP trials to provide buffer material which would help maintain the subject's interest in the programme while spreading out the

ESP trials enough to prevent fatigue effects. Another set of measurements could be made during the actual experiments to examine the physiology of psi guessing. The psychogalvanic skin reflex, pulse rate and respiration rate are all fairly easily recorded without great embarrassment to the subject. Even EEG recordings might be practicable if the necessary apparatus were available. The time relations between these variables, the stimulus and the guess might also be revealing and would require no additional experimentation.

A series of conventional ESP tests should be carried out as well to test the effects of those variables generally found relevant in psychological experiments. The effect on scoring of different levels of motivation should be investigated: the subject should be tested when hungry, thirsty, and satiated, and with stimuli relevant and irrelevant to the ruling 'drive'; he should be tested with reward and with punishment. The effects of experimental acceptance or rejection by the experimenter and others, and of frustration could be examined similarly. The effect of 'set' should also be determined: not merely 'striving' versus 'casual' which Otani (5) used, but more specific sets relevant or irrelevant to the type of experimental stimulus. If no practice effects had been found the design of these experiments could be of the factorial type, but if the subject were 'learning to learn' these effects would have to be considered and would be likely to complicate the design. If learning had been found in the initial experiment, further study of the learning process should be valuable. We could try different types of stimulus, e.g. designs, verbal material, nonsense and meaningful—and different types of task, e.g. sequence or paired associate learning—under different conditions, e.g. massed or spaced trials.

These are some general suggestions for the research programme. It is difficult to know how long these would take to investigate and since the programme must remain flexible for modification in the light of our earlier findings, there is little point in trying to be specific here. Before summarizing my suggestions I will describe in general terms some apparatus which has, I think, a number of merits, but is not at all indispensable to my proposed experiments. The basic idea would be to record by means of keys and pens both the experimental stimuli and the subject's response directly on to the same moving paper tape. With a slow moving kymograph this would be quite economical and yet it would provide direct recording by the subject without any chance of observers' errors. It would also provide permanent and accurate records of the time relationships between stimuli and guesses. Its chief virtue would

be to free the experimenter and subject from the presence of security observers and from complicated security rituals, while giving the subject the interest and satisfaction of using a machine. Another advantage of such a method is the possibility of recording a number of physiological measures and the 'judgment conditioning' on the same tape with all the time relations stored for later study. This basic system could be quite easily elaborated to present the subject automatically with a previously determined sequence, and to provide means of accurately pacing the subject's calls where the effects of speed are being studied. The exact specifications for such a system would be simpler for GESP presentation, but doubtless a method for use with clairvoyant subjects could also be devised like, for instance, Webster's (11) apparatus.

SUMMARY

The aim of this proposed research programme is to improve the availability of psi for research. In doing this it calls on the generalizations and techniques of the psychologist. In particular it is proposed to discover if a subject can improve his scoring with repetition of the same series of target stimuli, and if he can be trained to guess more accurately and confidently. Further it is hoped to find clues to the more successful manipulation of the experimental situation by manipulating social and motivational factors and by study of the individual and his physiological responses.

THE CALCULATION OF THE *S* STATISTIC

A. R. Jonckheere suggested the following method of computation. Make out the table of frequencies on page 147.

For each cell in turn, multiply the frequency (in our special case always 1) by the sum of all frequencies in cells 'south-east' of it, ignoring its own row and column. Then, again ignoring its own row and column, multiply it by the sum of frequencies in the cells to the 'south-west' of it. (In the last row of cells the products are always 0.) Subtract the second product from the first, and sum the result for each cell to obtain *S*. To test the significance of the *S* value obtained, reduce it numerically by 1, divide it by its standard deviation, and enter the normal ogive probability table. If at least 10 runs are made, *S* behaves as if normally distributed. The standard deviation of *S* is obtained from the formula :

$$S.D._S = \sqrt{\frac{1}{18} \{ N^2(2N+3) - 2\sum m^3 - 3\sum m^2 \}}$$

		Runs through the series								
		1	2	3	4	..	j	m	m ²	m ³
Hits o per run .. k	0									
	1									
	2									
	3									
	4									
	..									
	k									
							ΣN		Σm^2	Σm^3

and the critical ratio from this :

$$Z = \frac{|S| - 1}{S.D._s}$$

If S is significantly large and positive there is a trend from 'north-west' to 'south-east' across the frequency table, if significantly large and negative there is a trend from 'south-west' to 'north-east' across the table. If S is not large enough to reach an acceptable criterion of significance, no consistent trend can be inferred. S is not a measure of the slope of the trend.

REFERENCES §

- (1) Jonckheere, Personal communication.
- (2) Kendall, *Rank Correlation Methods* (2nd edit.).
- (3) Kendall, *Biometrika*, 36, 1949.
- (4) McElroy and Brown, *Journal of Parapsychology*, 14, 1950.
- (5) Otani, *Journal of Parapsychology*, 19, 1955.
- (7) Pratt, *Journal of Parapsychology*, 13, 1949.
- (8) Rhine, *Journal of Parapsychology*, 12, 1948.
- (9) Hudgins, *Journal of General Psychology*, 8, 1933.
- (10) Stuart, *Biometrika*, 40, 1953.

(11) Webster, *Journal of Parapsychology*, 13, 1949.

(12) Fisk and Mitchell, *Journ. S.P.R.*, XXXVII, 1953.

ADDENDUM

Since this essay was submitted for the competition, the author's attention has been drawn to the following points.

* It is still necessary in fact to refer to the theory of probability in detecting an upward trend, but that is one stage further back.

† Dr Soal has kindly pointed out an error here. It should read : 'to get an overall picture we could sum the *S*'s, take the square root of their summed variances, and divide as before.'

§ The author would also like to apologise for the omission from the list of references of the interesting paper by Mr Scriven published in this Journal for June 1956. It contained several ideas similar to those put forward here but unfortunately the author's copy of that number did not reach him because of change of address until after the present essay had been submitted.

P. R. F. C.

CORRESPONDENCE

SIR,—A member of the Society, Mr R. J. Hastings, has carried out a very thorough examination of my critique of the work of the late Dr J. Hettinger (*Proceedings*, 177, XLVIII). As a result of his investigations, three errors in my work have come to light which I am anxious to place on record with the Society.

1. On page 38, for 6,576 read 6,674.

2. In some of the tests in *Exploring the Ultra-Perceptive Faculty* comparisons were made between items and items, not between items and pictures. My argument at the bottom of page 38, though expressed in terms of the latter type of comparison, applies equally to the former. However, I wrongly included in the multiplication by '4 or 5' all the 1,576 items used in the item-item tests. I should have counted only half of them, for the other half are effectively targets.

It follows that 'in the region of 30,000' (page 38) should read 'in the region of 25,000'. Further, on page 39, '600,000' should read '500,000', and '10 million' should read '7½ million'. As explained in the text, the argument at this point was based on some very rough guesswork, and it is not materially affected by the above corrections.

3. On page 48, the table showing annotations by Western, the figure 14 for the number of C hits on controls should be 15, and the totals should be altered accordingly.

None of these errors have any appreciable bearing on my conclusions. I would like, however, to apologise for their occurrence and to thank Mr Hastings for having drawn my attention to them.

CHRISTOPHER SCOTT

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London, W.8.

SIR,—A copy of your Journal for June has just come to my notice, and on p. 85 thereof I observe a reference to myself by Mr G. W. Lambert which is erroneous, and which I ask leave to correct. Reviewing Mrs Iremonger's scintillating book, *The Ghosts of Versailles*, Mr. Lambert says :

'In Chapters XVIII and XXI accounts are given of the attempts to explain retrocognition by assuming that one can travel backwards in time (J. W. Dunne), or forwards (W. H. W. Sabine). Mrs Iremonger has done a valuable service here, in publishing in Chapter XXI a note by Professor C. D. Broad, in which he deprecates any attempt to explain it by imagining that one can actually perceive either the past or the future.'

Now Mrs Iremonger's Chapter XVIII is a restatement by her of my article, 'Is there a Case for Retrocognition?', which appeared in the American S.P.R. *Journal* for April, 1950, and that article contains no attempt to explain retrocognition by assuming that one can actually perceive either the past or the future. On the contrary, in that article, in other articles, and in my book, *Second Sight in Daily Life*, I have never conceded retrocognition in Myers's sense, or in Dunne's sense, or in any other sense except that of memory. Likewise, I have consistently expressed my preference for the view that precognition is a 'memory' of our own individual future sense experience, and that it does not betoken a 'reaching out' to cognize a 'future event' (*Journal of the A.S.P.R.*, Vol. XLIV, p. 62). There is our memory of our past, and our 'memory' of our future, and nothing outside that has, in my view, ever been substantiated by anyone.

Possibly Mr Lambert was thinking of one of several suggestions which I have made in discussing the particular case of Miss Moberly and Miss Jourdain, namely, that they might have precognized the results of the researches they were going to undertake. But Mr Lambert's statement about me, above quoted, conveys something quite different.

W. H. W. SABINE

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COMMENT ON MR SABINE'S LETTER

Mr Sabine is right in his surmise that I was thinking of the suggestion that Miss Moberly and Miss Jourdain might have precognized the results of researches they were going to undertake. In the passage from my review which he quotes I made the word 'travel' go too far, and should have made the end of the first sentence quoted read 'or "remember" forwards (W. H. W. Sabine)'.

G. W. LAMBERT

OBITUARY

GILBERT MURRAY, O.M.

With the death of Gilbert Murray on May 20th, 1957, at the age of 91, England has lost its most famous Greek scholar, one whose voice and personality were known to many thousands who had never opened a Greek text. He was born at Sydney in 1866 of an Anglo-Irish family (his father was president of the Legislative Council of New South Wales). Coming to England at the age of 11, he was educated at Merchant Taylors' School and St John's College, Oxford, where he won virtually every distinction open to an undergraduate. In 1888 he was elected to a Fellowship at New College, Oxford; but so deep was the impression that his personality and promise had made upon his seniors that in the following year, at the ripe age of 23, he was offered, and accepted, the Chair of Greek at Glasgow. The young professor took his duties very seriously, and there followed ten years of work so strenuous that by 1899 his health had begun to suffer. He had already in his Glasgow period produced his *History of Ancient Greek Literature* (1897), a book which, though in many ways immature, revealed a fresh and adventurous mind at grips with the interpretation of the Greek classics. When illness now forced him to resign his professorship, he devoted himself to the major task of editing Euripides, and also to the living theatre in which he was passionately interested. A fruitful decade followed: the three volumes of the Euripides text appeared successively in 1901, 1904 and 1910; the same period saw the publication of *The Rise of the Greek Epic* (1907) and the beginning of the long series of verse translations of Greek dramas—extending from the *Hippolytus*, *Bacchae* and *Frogs* in 1902 to the *Knights* in 1956—as well as two original plays, *Carlyon Sahib* and *Andromache*. In these years he also gained much practical experience of the theatre, and formed enduring friendships with Granville Barker, Sybil Thorndike and Bernard Shaw

who caricatured him with affectionate malice as Adolphus Cusins in *Major Barbara*). His appointment by the Crown in 1908 to succeed Bywater as Regius Professor of Greek at Oxford marked the beginning of a new epoch both in his personal life and in the history of Greek studies in England. His fame as a lecturer quickly spread far beyond the bounds of the university, and his reputation as a scholar was confirmed by the appearance of two small but brilliantly written books which have had a wide and lasting influence, *Euripides and his Age* (1913) and *Four Stages of Greek Religion* (1912, republished with an additional chapter in 1925 as *Five Stages of Greek Religion*).

The war of 1914-18 diverted a large part of Murray's prodigious energy into new channels. The growth of nationalism and the growing threat of international conflict had long troubled him; as early as 1900 he contributed to *The International Journal of Ethics* a devastating paper entitled 'National Ideals, Conscious and Unconscious' (reprinted in *Essays and Addresses*, 1921). But it was the war which convinced him that some constructive action was urgently needed if Europe was to be saved from tearing itself to pieces. His judgment on the events which led up to the war is to be found in *The Foreign Policy of Sir Edward Grey* (1915). When it was over he threw himself with missionary zeal into the work of rebuilding a broken world. He was one of the principal architects of the League of Nations Union, of which he was Chairman from 1923 until 1938; and its educational offshoot, the Council for Education in World Citizenship, was very largely his personal creation. During the inter-war years much of his time was occupied in working for the League of Nations at Geneva, where for eight years he presided over the Committee for Intellectual Co-operation, a remarkable and devoted body whose membership included Einstein, Bergson and Madame Curie. He also stood several times for Parliament in the Liberal interest, though without success. Yet he found time in those distracted years to produce his Harvard Lectures on *The Classical Tradition in Poetry* (1927), a book on Aristophanes (1933) and an Oxford text of Aeschylus (first edition, 1937). His retirement from active teaching in 1936 brought no abatement of the energy which he devoted to the twin causes of international justice and Greek studies. The second war and the bitter disappointments which followed it, far from destroying his faith in collective international action, only strengthened his conviction of the need for it; he saw these events in the long perspective of history, and patiently in old age set about building the United Nations Association, of which he was joint President with his old companion Lord Cecil down to the time of his death. In

scholarship too he remained active to the last: his book on Aeschylus was published in 1940, a volume of collected *Greek Studies* in 1946; several new translations followed; and in his ninetieth year he was still toiling with the help of younger friends to produce the revised (and greatly improved) edition of his Aeschylus text which appeared at the end of 1955.

Murray had throughout his life a highly personal conception of the scholar's task. He defined it in his first Presidential Address to the Classical Association: 'the Scholar's special duty is to turn the written signs in which old poetry or philosophy is now enshrined back into living thought or feeling. He must so understand as to relive.' In the Oxford of fifty years ago this was a revolutionary doctrine. The serious Oxford scholars of that time were mainly engaged in exact textual studies like those of Bywater, A. C. Clark and T. W. Allen; the rest were largely occupied in teaching their pupils to put conventional English verse into more conventional Greek iambics or Latin elegiacs. Murray was no enemy to either sort of scholarship; he was himself an ingenious and subtle (sometimes over-subtle) textual critic, and also a master in the traditional English art of 'composition'. But his instinct told him that neither of these things was enough if the knowledge and love of Greek was to survive in the new and unfriendly climate of the twentieth century. The scholar must also (if he can) relive, and cause others to relive, an ancient piece of human experience.

And Murray's essential personal gift was his ability to do precisely this. His lectures were memorable, not merely for the delicate art with which they were composed or the beauty of the voice in which they were delivered, but because they were a communication of experience; it was this that gave them their quite extraordinary quality of immediacy. To hear Murray read aloud and interpret a passage of Greek poetry brought to successive generations of his students the intoxicating illusion of direct contact with the past, and to many of them a permanent enlargement of their sensibility. The same power of reliving the past can be felt in his best critical work, for example in the sustained effort of imaginative reconstruction which gives substance and credibility to *The Rise of the Greek Epic*.¹ And it determined his personal approach to the ancient drama. For Murray a play was first and foremost a piece of theatre to be enjoyed and criticized as such, and only secondarily a document to be analysed in the study. And

¹ Is it fanciful to see a connection between Murray's exceptional power of 'reliving the past' and his exceptional success in telepathic experiments where the problem set was most often that of reliving a past experience, whether drawn from life or from literature?

here his intimate familiarity with the contemporary stage gave him an important advantage over purely 'documentary' scholars. His acute sense of the theatre can be felt as an influence—sometimes misleading, but often illuminating—on almost every page of his text of Euripides. And it helps to explain the unparalleled success of his translations, of which at the time of his death nearly 400,000 copies had been sold. Murray had 'relived' each play that he translated; that is why, as Shaw expressed it, 'they came into our dramatic literature with all the impulsive power of original works'. It is true that in the process of reliving certain qualities of the original vanished, and were replaced by others which it possessed only by remote implication if at all; but Murray could plead, as Edward Fitzgerald did, that 'at all cost a thing must *live*, with a transfusion of one's own worse life if one can't retain the Original's better'. Murray's versions were alive, they made moving theatre, and they restored Euripides to the stage as no English translator had ever done before. In recent years fashion has turned against their luscious rhythms and decorative Georgian style, but for sheer technical accomplishment they still have no rival among our translations from the Greek since Pope's Homer.

The immediate impression made by Murray's personality was one of gentleness, serenity, effortless control and perfect balance. (His mental sense of balance had its physical counterpart: up to an advanced age he was prepared to demonstrate the latter by walking up a ladder without using his hands.) Whether his serenity was the gift of nature or the reward of self-discipline is open to doubt. Shaw, who had known a younger Murray, described 'Adolphus Cusins' as 'a most implacable, determined, tenacious, intolerant person who by mere force of character presents himself as—and indeed actually is—considerate, gentle, explanatory, even mild and apologetic, capable possibly of murder, but not of cruelty or coarseness'; and there is perhaps a truth behind the exaggeration. Certainly the mature Murray combined an exquisite courtesy to his opponents with an unswerving loyalty to his own purposes. He was tolerant with the tolerance of magnanimity, not of blindness, just as his idealism was based on a realistic and not very complimentary assessment of average human nature. As far back as 1900 he had written that if you scratched any civilized European deep enough you would find a savage; and so, while he fought always for the best, the worst could not surprise him. His ultimate aim was always peace, but he knew too much of the evil forces in man to be either a pacifist or an 'appeaser'. He knew also that martyrs are often wrongheaded, and that their blood is only rarely the seed of a Church; but he had an instinc-

tive sympathy with them, as with all who stand for conscience against authority. To their suffering, and to all suffering, his response was instantaneous and generous, as refugee scholars from many countries have cause to remember. Yet he was hardly a typical humanitarian; he had, for one thing, an irrepressible sense of the ridiculous. And though he chose out of personal taste to abstain from meat, wine and tobacco, he was anything but a killjoy; he loved company, and was a brilliant raconteur and a most accomplished host, expert at drawing out the best that each could contribute and at making the youngest and shyest feel at ease at his table where poets and actors, cabinet ministers and foreign savants, rubbed shoulders with raw undergraduates. Despite political and family disappointments, he was among the happiest people the present writer has known: he had attained, in his own words, 'that which is the most compelling desire of every human being, a work in life which it is worth living for, and which is not cut short by the accident of his own death.'¹ Whether he is to be ranked with the greatest scholars depends on one's conception of scholarship; but that he was a truly great man no one who knew him could doubt. The world's opinion of him was signified in his lifetime by the O.M. and the Pour le Mérite, and after his death the ashes of this unrepentant rationalist and agnostic² were placed in Westminster Abbey—a gesture that would have both touched and amused him.

E. R. DODDS

Dr Gilbert Murray was for many years a close friend of my parents, Dr and Mrs A. W. Verrall. He was often at our house, and I particularly remember a summer holiday we all took together in Switzerland, when I was about sixteen. A year or two later my parents spent the summer in Surrey, where we had Dr and Lady Mary Murray for our near neighbours. He was a gay and charming companion, always ready to give a hearing to my crude, youthful opinions. If he dissented from them, it was with the manner of one who happens to differ from a friend. He was quite without pomp, or any sort of false dignity. He was a considerable gymnast in those days, and I have recorded elsewhere how I saw him hanging head-downwards from the trapeze at the Swiss hotel in which we happened to be staying. He would also

¹ *Essays and Addresses* 13. It gave him particular pleasure that on the occasion of his ninetieth birthday the Gilbert Murray Trust (25 Charles Street, W.1) was created to further the two great objects of his work, Greek studies and international co-operation.

² Murray's view of religion is most fully stated in a little book entitled *Stoic, Christian and Humanist* (1940, reprinted with a new introduction in 1950).

excite my envy by dancing a sort of hornpipe which called for exceptional muscular strength, and a good sense of balance. I strove in vain to emulate him.

He and my parents had much in common, not only scholarship, but wide literary and dramatic interests, and politics ; they were all stalwart liberals. With my mother he also shared an interest in psychical research, and she was one of the first people to bring to the Society's notice his remarkable telepathic gift. Her paper reporting and discussing his experiments was read at a meeting of the Society in February 1916, and published in S.P.R. *Proceedings*, Vol. XXIX, pp. 64-110. In the previous year Dr Murray had himself referred to his experiments in his Presidential Address, *Proc.* XXIX, pp. 46-63. A further Report by Mrs Henry Sidgwick was published in *Proc.* Vol. XXXIV, pp. 212-274, with an important Appendix in the same Volume, pp. 336-341. Some years later in May 1931 my husband and I were able to witness some experiments when we were staying with Dr and Lady Mary Murray in their house on Boar's Hill, Oxford. A Report of these experiments appeared in the Society's *Journal*, Vol. XXXII, pp. 29-38. Dr Murray also gave some further account of his experiences in his second Presidential Address, delivered in May 1952, and published in S.P.R. *Proc.* Vol. XLIX, pp. 155-169. From this Address I shall later quote a few sentences.

All the experiments followed the same general pattern, though the conditions varied a little. A group of people assembled in one room and in Dr Murray's absence a scene or incident was selected for transmission, briefly described by the person who selected it (the agent), and noted in writing, usually by another member of the company. Dr Murray then came into the room and described his impressions. Sometimes he held the agent's hand, but many successful results were obtained without contact. Although the agent was in many cases a member of Dr Murray's own family, he was successful with a number of other people. Clearly the percipient was in his case the essential partner. On some occasions details were correctly given by Dr Murray which had been in the agent's mind, but had never been spoken aloud. This point is important, because the only alternative hypothesis to some para-normal faculty which can be put forward with any plausibility is hyperaesthesia of hearing. The results cannot be attributed either to chance-coincidence or *normal* hearing. On this last point all observers are agreed, and my husband and I were at some pains to establish it in the experiments we ourselves witnessed. In his first Presidential Address Dr Murray was himself inclined to stress the hypothesis of hyperaesthesia, mainly, it would seem, from a

reluctance to make a paranormal claim, but later he admitted that the source of his knowledge seemed to be mainly telepathic, though telepathy might make use 'of real sights, sounds, smells, memories, to reach its goal'. On this question of hyperaesthesia I will quote from a letter contributed to the *Journal*, Vol. XXXII, p. 161, by the late Lord Rayleigh, commenting on my Report of the experiments I had witnessed: 'I have often read about hyperaesthesia in this connection. But what is really meant by this expression? And have we any positive evidence pointing to such a supposed faculty intervening in telepathic tests?' Aural surgeons, says Lord Rayleigh, commonly make tests of hearing with a tuning-fork, and he wonders whether they have ever encountered anything which affords evidence of hyperaesthesia. 'I doubt it. And without such evidence hyperaesthesia has really no definite meaning. In my experiments on seeing lights near a magnet I tried to investigate the question for sight, and failed to find any indication that there are visual "hyperaesthetes".'

In favour of the telepathic hypothesis I will also quote a passage from Dr Murray's second Presidential Address, *Proc.* Vol. XLIX, p. 163: 'Of course the personal impression of the percipient himself is by no means conclusive evidence, but I feel there is one almost universal quality in these guesses of mine which does suit telepathy and does not suit any other explanation. They always begin with a vague emotional quality or atmosphere: "This is horrible, this is grotesque, this is full of anxiety"; or rarely, "This is something delightful"; or sometimes, "This is out of a book", "this is a Russian novel", or the like. That seems like a direct impression of some human mind. Even in the failures this feeling of atmosphere often gets through. That is, it was not so much an act of cognition, or a piece of information that was transferred to me, but rather a feeling or an emotion; and it is notable that I never had any success in guessing mere cards or numbers, or any subject that was not in some way interesting or amusing.'

Admittedly it would have been an advantage if it had been possible in the midst of Dr Murray's very busy life, to carry out these experiments more systematically. But in spite of their limitations they are among the most interesting and important contributions to the Society's records.

H. DE G. S.

Gilbert Murray joined the Society in 1894 and became a member of Council in 1906. He was President for the years 1915 and 1916 and again on completion of the Society's seventieth year in 1952. At the time of his death he was the senior Vice President.